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A Critical Study of the Action of a Diamond in Ruling Lines Upon Glass.

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In offering a communication upon the subject indicated by the title of this paper, I am not unmindful of the fact that I enter a field in which I acknowledge a master. Since the death of the incomparable Nobert, Mr. Fasoldt, of Albany, stands easily first in the art of fine ruling. I desire to repeat here the reply which for the past three years I have invariably made to inquiries for test-plates from my own machine—viz., that with Mr. Fasoldt's special facilities for this class of work he can, I have no doubt, produce far better results than it would be possible for me to obtain by chance efforts. I have thought it better to confine my attention to another equally important problem—viz., an attempt to obtain copies of the Imperial Yard and of the Metre des Archives, at the temperatures at which they are standard, to subdivide these units into aliquot parts and then to obtain a microscopical unit whose subdivisions should be so nearly equal that the microscope would fail to reveal the difference. The first part of this work has been mainly completed. Two independently-obtained copies of the Imperial Yard yield nearly identical values for the length of this standard unit. Three independent comparisons with the Metre des Archives agree within very narrow limits in defining the absolute length of the metric unit, both at 32 and 62 degrees Fahrenheit. The subdivision of these units into aliquot parts—the yard into inches and the meter into centimeters—has been so far completed that any er-

rors which may remain will not affect the microscopical unit sought. With regard to the exact subdivision of these units, I can only report progress.

Notwithstanding this abandonment of attempts to produce test bands of the Nobert pattern, I have recently taken up the subject again, somewhat with the view of testing the claim of Mr. Fasoldt that he has succeeded in ruling lines one million to the inch, and especially by the claim that the existence of a spectrum in the bands is an evidence of the reality of the separate lines. The latter claim does not appear to be well founded. Aside from being at variance with theory, it can easily be disproved experimentally.

Before proceeding further with this investigation, I beg to refer to a theory proposed by the writer in a paper presented to the American Academy of Arts and Sciences, in 1875, in relation to the method which Nobert may possibly have employed in the production of his test-plates. Briefly stated, this theory is that the lines composing Nobert's bands are produced by a single crystal of the ruling diamond, whose ruling qualities improve with use. In the light of subsequent experience this theory may be stated in the following way: When a diamond is ground to a knife edge, this edge is still made up of separate crystals, though we may not be able to see them, and a perfect line is obtained only when the ruling is done by a single crystal. When a good knife edge has been obtained the preparation for ruling consists in finding a good crystal. Occasionally excellent ruling crystals are obtained by splitting a diamond in the direction of one or more of the twenty-four cleavage planes which are found in a perfectly-formed crystal. A ruling point formed in this way is, however, very easily broken, and soon wears out. Experience has shown that the best results are obtained by choosing a crystal having one glazed surface and splitting off the opposite face. By grinding this split face, a knife edge is formed against the natural face of the diamond which will remain in good condition for a long time. When a ruling crystal has been found which will produce moderately heavy lines of the finest quality, it is at first generally too sharp for ruling lines finer than 20,000 or 30,000 to the inch, even with the lightest possible pressure upon the surface of the glass. But gradually the edges of this cutting crystal wear away by use

until at last this particular crystal takes the form of a true knife edge which is parallel with the line of motion of the ruling slide. In other words, when a diamond has been so adjusted as to yield lines of the best character its ruling qualities improve with use. If Nobert had any so-called "secret," I believe this to have been its substance.

The problem of fine ruling consists of two parts—first, in tracing lines of varying degrees of fineness; and, second, in making the interlinear spaces equal. The latter part of the problem is purely mechanical, and presents no difficulties which can not be overcome by mechanical skill.

It will be the aim of the present paper to describe the more marked characteristics of lines of good quality ruled upon glass, and to illustrate these characteristics by corresponding specimens. To one who is familiar with Nobert's bands, a perfect line need not be described. It is densely black, with at least one edge sharply defined. Both edges are perfectly smooth. Add to these characteristics a rich black gloss, and you have a picture of the coarser lines of a perfect Nobert plate. How are those lines produced? In the study of the action of a diamond in producing a breaking fracture in glass, the microscope seems to be of little service, but we can call it to our aid in the study of its action in ruling smooth lines.

One would naturally suppose that a line of the best quality would be produced by the stoppage of the light under which it is viewed by the opaque groove which is cut by the ruling diamond. Without doubt this is the way in which lines are generally formed. But it is not the only way in which they can be produced. An examination under the microscope will reveal the fact that in some instances, at least, a portion of the glass is actually removed from the groove cut by the diamond; and that the minute particles of glass thus removed are sometimes laid up in a windrow beside the real line, as a plow turns up a furrow of soil. On the finest plate I have ever produced every line remained in perfect form for about two months. I then first noticed a tendency on the part of some of the single lines to disintegrate, while the lines ruled in closer bands seemed to retain their good qualities. This disintegration finally became so marked that, as an experiment, I removed the cover and cleaned

one-half of the surface of the glass by rubbing with chamois skin. The difference in the appearance of the two halves is now very marked. Above, the dense black lines remain. Below, a ragged abrasion of the surface of the glass has taken place. Above, the furrowed lines as originally formed are preserved; below, there is a coarse scratch. It may be said that the action in this case is accidental and abnormal. In reply I can say I have prepared plates which show that the particles of glass removed take four characteristic forms: (a) They appear as chips scattered over the surface of the glass. (b) They appear as particles so minute that when laid upon a windrow and forming an apparent line they cannot be separated under the microscope. (c) They take the form of filaments when the glass is sufficiently tough for them to be maintained unbroken. (d) They take a circular form.

I regret that three of the most striking specimens were broken in mounting. In one, a perfect line about one thirty-thousandth of an inch in width was formed with a clear space between it and the groove cut by the diamond. There was not a single break in these filaments from beginning to end, but at nearly equal intervals of about one-hundredth of an inch half-knots were formed similar to those formed in a partially-twisted cord. By rubbing the surface at one end, these filaments were broken up. For the most part they assumed a semi-circular form, but some of them maintained their thread-like form and became twisted together in the most intricate fashion.

In the third specimen, which was broken in mounting, the glass removed took a spiral form like the spiral chips from steel when turned in a lathe. A projecting crystal of the diamond caught these spirals and carried them unbroken to the end of each line, leaving them a tangled mass of threads. Even after they were protected by a cover-glass cemented to the surface, many of these spirals remained intact. Judging by the difference in focus of the various parts, the height of the mass, before the plate was covered, must have been one five-hundredth of an inch.

The same ruling crystal may produce smooth lines or either chips or threads, according to the motion of the diamond, as may be seen by examination of the accompanying rulings. In these

plates one-half of the lines of the bands are ruled by a forward motion and one-half by a backward motion of the diamond. Chips may be formed in ruling bands of very fine lines, as illustrated in the bands of lines twenty-four thousand to the inch.

It must not, however, be supposed that lines of the best quality always present the appearance described above. While it is exceedingly rare that lines appear as well after the surface of the glass has been rubbed as before, many instances have occurred within my experience in which the difference, especially in fine lines, was not particularly noticeable. According to the limited evidence at hand, the coarser lines of Nobert's bands present some of the characteristics which I have described. I have restored two of these plates, in which the lines had become nearly obliterated by some kind of condensation under the cover-glass. In one, the quality of the lines was not much affected by the operation of cleaning, but in the other the dark gloss which characterizes the heavy lines of nearly all of Nobert's plates was entirely destroyed. The finer lines, however, were much less affected than the coarse ones.

Lines of the character thus far described are evidently unsuited to the ordinary works of the microscopist. It is my experience that lines which are the most symmetrical in form and the most beautiful in appearance are produced indirectly rather than by the direct action of the diamond in cutting a groove in the glass. They can be protected to a certain extent by a cover-glass, but they are liable to undergo changes which will affect their original structure. Except for purposes of investigation, therefore, there is no advantage to be gained by ruling lines of this character.

Three conditions must be fulfilled in the production of lines having a permanently good character:

1. The glass must be tough. There is a marked difference in the character of the filaments produced, and, to a certain extent, of the lines themselves, yet the conditions under which the lines in the series of plates illustrating this paper were ruled were the same in nearly all of the plates—i. e., the same diamond was used; its setting remained unchanged, and there was no change in the pressure of the diamond upon the surface of the glass. I may add also, that I have in my collection several other plates which were ruled

especially to test the question of the requisite quality of the glass. They all agree in giving evidence that glass of a given quality will always yield lines of nearly the same quality—the ruling crystal remaining the same and in the same position.

2. The greatest difficulty encountered in setting a ruling crystal is to obtain one which will rule lines of the required quality which will retain their form after the surface of the glass is rubbed. The crystal with which nearly all the plates of this series were ruled was only obtained after a search continued at intervals through several weeks. Sometimes a diamond which will rule good light lines will not produce good heavy lines, and *vice versa*. According to my experience it is better to have a special diamond for each class of lines desired, though the diamond with which the present series of plates was ruled seems well adapted to every kind of work required except, perhaps, the production of the finest bands. An examination of plates illustrates the wide difference in the character of lines ruled with the same diamond, after the edges of the ruling crystal have been worn smooth. In one there are two sets of lines, side by side, in one of which the surface has been rubbed, and in the other of which the lines have been left undisturbed. The difference is very marked. It may be said here that the surface of a ruled plate should always be cleaned by rubbing in the direction of the lines only, never at right angles to the lines. It will often happen after sharp rubbing that the lines appear ragged, when the difficulty is that the chips have not all been removed from the grooves. Rubbing with Vienna lime, moistened with alcohol, will usually complete the cleaning satisfactorily.

3. After a crystal has been found which will fulfill the conditions of producing a line which will bear cleaning there still remains a difficulty which will only be revealed after the lapse of considerable time. This is well illustrated in one plate, in which the lines were as perfect as could be desired for several days after they were ruled. The lines of the band are now completely broken up. Evidently they were under a strain, which finally became so great that resistance to rupture became impossible. This, however, is an extreme case. Generally the lines simply enlarge at certain points. Usually the termination of the enlargement occurs at irregular dis-

tances along the lines, and it is nearly always very sharply defined. The most curious action of this kind which has ever come under my notice is where the lines have broken up into a form something like the strand of a heavy rope.

The process of setting a diamond is as follows: The holder has the means of adjustment in three planes: (a) An adjustment in a horizontal plane; (b) an adjustment in a vertical plane; (c) an adjustment in a plane at right-angles to the ruled lines. It is my practice to begin by giving the knife edge of the diamond considerable inclination to the line of motion of the ruling slide. I then rule a series of single lines at different known angles of inclination, care being taken to pass the line of parallelism. An examination of the character of the lines thus ruled will enable one to determine within narrow limits near which one the knife edge is set parallel with the slide. After a fair line has been obtained in this way, a sharp crystal is generally found by tilting the diamond in a vertical plane, though it will often be found necessary to make the third adjustment mentioned. Sometimes the cutting crystal is lost after ruling a few lines, but generally good results can be obtained after a constant service of weeks, and even months. A crystal is lost either by being broken off or by being worn out. When a crystal has been lost it need not be concluded that the diamond needs sharpening. It is only necessary to find a new crystal, an operation requiring patience rather than skill.

It should be stated that while this theory of individual-cutting crystals seems to be the true one, I have never been able to detect them by an examination with the microscope. It is only by their behavior that their existence can be recognized.

One of the most severe tests of the ruling qualities of a crystal consists in producing, without fracture, heavy lines which cross each other at a small angle of inclination and which will receive graphite without interruption of continuity at the intersection. Lines ruled at right-angles and forming small squares afford a better test than parallel lines. In one plate presented the curved lines formed by the intersection of straight lines are nearly perfect in form, and they hold the graphite quite as well as the original lines. In another plate I have attempted the representation of the nucleus of a

comet. The filling is not quite as perfect as in the other plate, but this is due to the quality of the glass. Attention is called to the granular structure under a moderately high power. I have found rulings of this form to be an excellent test of the quality of the glass required for receiving the best lines. In general, the first filling of the lines is the most perfect. One plate affords an illustration, exceedingly rare, of lines which receive the lines equally well after repeated fillings. Lines as fine as fifty thousand to the inch very readily receive the graphite. The limit beyond which it seems impossible to go may be placed at about one hundred thousand to the inch.

A few words may properly be added here with regard to the protection of ruled lines. When lines are formed by a true groove in the glass, it is better that they should remain unprotected. But when the lines are formed in the manner illustrated by the plates of this series, the quality of the lines in the end is pretty sure to deteriorate whenever there is an actual contact of the cover-glass with the slide. I have made serious efforts to overcome this difficulty, but with only partial success. Slides mounted with gutta-percha rings generally remain in good condition for a long time, especially if, after expelling the air as far as possible by heat, a ring of white wax cements the rim of the cover-glass to the slide. But even with this precaution there is no certainty of final preservation. If it should be found that the brass slides of this series are convenient in manipulation, their adoption can be recommended, since they entirely obviate this difficulty. They are made in the following way: A hole having been made in the center, a flange is left one two-hundredths of an inch in thickness. The cover-glass is then cemented to the surface of the brass, and the rulings are made on the under side. The protection is made by dropping upon the ledge of brass a rather thick circle of cover-glass, which is held in position by a circular brass wire.

After this digression, I return to the consideration of the credibility of Mr. Fasoldt's claim that he has succeeded in ruling lines one million to the inch. At this point it is only fair to say that until recently I have shared in the general incredulity with which Mr. Fasoldt's claim has been regarded. Indeed, I still think he has

placed the limit just a *trifle* too high. But if the limit is reduced one-half, I am by no means sure but that it may be reached. Possibly it may have been already reached.

But what evidence have we that it is possible to see single lines of this degree of fineness, granting that it is possible to produce them? The answer to this question involves another inquiry—viz., has the microscope reached its highest visual possibilities? Here again it is necessary to draw a sharp distinction between visibility and resolution. In the matter of limit of resolution it must be admitted that little or no progress has been made since the resolution of Nobert's nineteenth band. The distinguishing feature of Nobert's lines is a certain boldness which enables them to be photographed, and it is to photography, supplemented by the statement of the maker, that we owe the certainty of the resolution of the nineteenth band. But all attempts to go beyond this band, even with Nobert's later plates, have proved failures. I cannot learn that anyone has yet succeeded in photographing a Fasoldt plate as high as 100,000 to the inch. Certainly various attempts which have been made with bands of my own ruling higher than about 70,000 have not been successful. There are several Nobert plates of the new pattern in this country. They run as high as 240,000 lines to the inch, but who has gone beyond the number of lines in the nineteenth band? With great respect for the honest belief of several microscopists who claim to have resolved Fasoldt's bands as high as 152,000 to the inch, I must yet hold to the opinion that in no case has the resolution been proved by a test which will be generally accepted by microscopists. There is one test, and only one, which is absolutely decisive—viz., the one originally proposed by Nobert, that of ruling a definite number of lines in a band of given fineness and keeping the number secret until the microscopist could give the correct count, not merely in one instance but in several. Even here we must depend upon the honesty of the maker in revealing the correct count. Has the correct count been made in any Fasoldt plate as high as 100,000 to the inch? I think not. Has it been done with any band of my own ruling of the same degree of fineness? No. Let us marshal the evidence, pro and con, offered by experience.

Mr. Fasoldt's finest bands present a perfectly smooth and uniform surface. They have well-defined limits and the width of the bands is what it should be by the number of lines claimed to be ruled. (b) According to present experience single lines can be ruled several degrees finer than I have been able to detect under the microscope. About four years since I sent to Professor J. Edwards Smith a ruled plate with a statement of the number of bands, accompanied with a description of the same. Soon after, I received a letter from Professor Smith saying there must be some mistake in the description, as he was unable to find two of the bands. I replied that the bands were certainly ruled, and that I thought I could convince him of that fact. I therefore requested him to re-examine the plate with the greatest care, and if he was still unable to find the bands to return the plate to me. After a vain endeavor to discover them the plate was sent to me. I removed the cover, filled the lines with graphite, remounted the slide, and returned it to Professor Smith. Not only had the invisible bands become visible, but the separate lines, with an interlinear space of 1-80,000 of an inch, were easily seen. Now, when Professor J. Edwards Smith, an acknowledged expert in the manipulation of the microscope, is unable to find lines which are really in the center of the field of the microscope, I suspect that other observers may find a similar difficulty. Among the plates presented is one series which were ruled to illustrate the possibility of producing lines which really exist, but which are invisible under the microscope. On one plate there are two sets of lines, one set on the slide and the other on the under side of the cover. Between the bands, 10,000 and 24,000 to the inch, the entire intervening space is filled with a continuous series of bands, 24,000 to the inch. I have not been able to see the lines of the last band. In another plate there are a series of bands containing twenty-one lines each, the entire linear space being 1-2,000 of an inch. The first eleven lines are ruled with a forward motion of the diamond, and the second ten lines are ruled with a backward motion. The last two bands are preceded by heavy finding lines. Each of the last three bands are followed by bands 24,000 to the inch. I think it will be found difficult to see the lines of the last two bands under any illumination at present in use, and yet I am

confident that the lines exist. I found my belief upon two bits of evidence: First, the pressure of the diamond upon the glass was sufficient to produce the lines. With considerable less pressure there would still have been a constant contact between the diamond and the glass. Second, I saw them ruled through the sense of hearing. When a diamond does its very best work it produces a sharp, singing tone, which is audible at a distance as great as twelve inches. This singing tone I distinctly heard for every line ruled. It is even more marked in ruling the finest lines than in coarse ones. I have two singing diamonds, or rather two diamonds with singing crystals, and these two are the ones with which I have done my best work.

The argument against the visibility of single-ruled lines which can not be seen with the present means at command, even if within the limits of possibility, considered in a physiological sense, is in one respect a sufficient answer to the evidence offered in favor of their existence. This evidence, while not exactly negative in its character, is yet not sufficiently conclusive to be regarded as coming under the head of proof through the medium by which the existence of any fact is attested—viz., the medium of some one of the senses. But may it not be true that we have not yet reached the fulfillment of the conditions necessary to visibility? It certainly cannot yet be safely asserted that it is impossible to see a material particle which has, in one direction, a magnitude not exceeding 1-500,000 of an inch. Photography offers the evidence, somewhat negative in its character, that the limit of visibility is reached with lines having a width of about 1-200,000 of an inch. Lines of this width are the finest that have ever been photographed. But the most conclusive evidence against the certainty of being able to produce lines as fine as 500,000 to the inch consists in the fact, repeatedly proven in my own experience, that lines which appear to be excessively fine often have a real width two or three times as great as they appear to have, as has been proved conclusively by filling the lines with graphite, which brings out the real limit. This phenomenon will come out again in connection with the subject of resolution.

I have already stated my belief that the limit of resolution has been so nearly reached that, though it is quite possible under a combination of favorable circumstances to obtain a resolution a lit-

tle beyond 113,000 to the inch, the uncertainty which must always attend observations of this character is so great that the certainty of resolution can not be safely asserted. In consideration of this uncertainty and of the fact that so little progress has been made in resolution compared with the recent advance in the construction of objectives, I beg to propose as a test the visibility of single-ruled lines in place of the resolution of these lines in close combination. Instead of bands of lines of the Nobert pattern, I propose a series of bands, each having the same interlinear unit, but with the lines of each successive band finer than those of the preceding band. The space between the lines should not be so great as to interfere with their easy detection, nor so small as to require any effort in resolution. One mikron is a convenient unit. A heavy line should precede the band in order to facilitate finding it.

According to my own experience there are four facts which must always throw grave doubt upon any reported case of difficult resolution:

1. It is well known that by the manipulation of the light, every other condition remaining the same, it is possible to vary the apparent number of lines in a given band of coarse rulings. Can anyone offer a reason why there should not be the same difference with bands of fine lines closely ruled?

2. I have many times ruled bands of lines with the interlinear spaces distinctly marked, but in which each line was in reality considerably wider than the space between the lines, as I have proved by extending single lines beyond the others and filling them with graphite. The only explanation of this singular fact which I can suggest is that the diamond may possibly cut square down at one edge of the line and for the remainder of the line produce only an abrasion of the surface of the glass, which is so slight as not to interfere with throwing up a furrow upon the remaining portion.

3. Lines of a given depth appear finer when closely ruled in bands than they do in single lines.

4. I add another observation with some hesitation, since I have not been able to prove its truth beyond peradventure. I have often, but not always, found that when single lines, apparently invisible, are placed in close combination in bands, they not only form a

visible band, but a band capable of apparent resolution into separate lines. Can anyone offer a reason why we can see in combination what we can not see as separate parts? Of course I shall be at once reminded by the astronomer that it is much easier to pick up a cluster than to see scattered stars of the same magnitude. But when it is once found, the separate stars composing it are no more easily seen than stars of the same magnitude more widely scattered. I offer this observation in a tentative way, since it has, if true, an important bearing upon the question of the ultimate limit of resolution. Among the accompanying plates is one that illustrates the statement here made. This plate consists of a series of bands, 12,000 to 24,000 to the inch, each preceded by a heavy finding line. The lines of each successive band are finer than the preceding. The last two bands were ruled with the same pressure of the diamond as the fourth band preceding. The intervals at which they were ruled are 1-80,000 and 1-200,000 of an inch. I do not by any means vouch for the existence of the separate lines, yet the bands are smooth, and there is a distinct difference in the appearance of the two halves of the 80,000 band, the first having been ruled with a forward and, the second with a backward motion of the diamond. The corresponding single lines of the fourth band preceding are wholly invisible. This plate seems to show that the visibility of the lines in bands depends somewhat on the narrowness of the interval between the lines, since the lines of the same degree of fineness with an interval of 1-24,000 of an inch can not be seen.

It is obvious that this whole question of resolution needs the most careful consideration and investigation, since it bears an intimate relation to the limit of visibility of single particles of matter. Mr. Hitchcock, in a recent number of his *Journal*, has made the claim that resolution has to a certain extent ceased to be a test of the quality of an objective. I suspect that this claim will be found to have some foundation in fact. For the last ten years we have only the assertion of resolution, without doubt honestly made, but yet unaccompanied with the proof. It is time that the proof should accompany the assertion. I insist that simple vision does not afford the required proof.

Now we must face this question as honest inquirers after truth. There is a limit which theory places to resolution with objectives

of given resolving power, not to visibility, as has been frequently stated. Before we can safely assert that observation has gone beyond theory, we must be prepared to offer evidence which can be placed upon record, can be discussed deliberately, can be weighed impartially in the balance with counter evidence, and can still stand unimpeached. Do you say that is hardly worth the trouble? I reply that the issue here raised comes to the surface in one form or another at almost every point in physiological and pathological investigations. It will do no harm to recall the number of times it has at this meeting stood as a sentinel at the entrance to the temple whose mysteries we are seeking to explore. Has not the question so tersely put by Dr. Gleason at the Elmira meeting of this Society: "Do we see what we see, or don't we see what we see, or do we see what we don't see?" been the stopping-place of more than one important issue raised at this meeting? I hope I do not need to say that I have no personal ends to serve in an inquiry in which I happen to be a personal factor. Let us then have a test which will forever set at rest this vexed question of resolution. I submit for your consideration the following outline of a test which I venture to think will be sufficient and conclusive: Let Mr. Fasoldt rule three plates under as nearly the same conditions as possible, except in the number of lines in the different bands of each plate. Let him label each plate and accompany it with a full description of the number of lines in each band. Let these plates be sent to any gentleman in whom the great body of microscopists have confidence as eminently qualified to conduct an investigation of this sort, such as Prof. H. L. Smith of Geneva or Col. J. J. Woodward of Washington. Let whoever receives the plates remove the labels of Mr. Fasoldt and put in their place labels whose signification is known only to himself. Then let the gentlemen who think they have resolved 152,000 lines to the inch take the plates, make their count of the lines in each band, and send in their report. Let the plates also be photographed, and let the number of lines be counted; then let the results of these investigations be published. If all substantially agree in the count, this will end further discussion.

The limit of visibility of a single particle of matter under the microscope bears an intimate relation to the limit of naked-eye visibility.

My attention was first called to the smallness of this limit by an accidental circumstance. I had ruled a micrometer upon a thin cover-glass, consisting, as I supposed, of moderately coarse lines. After several vain attempts to discover traces of the lines ruled, I chanced while holding the glass at a certain angle with respect to the source of light to breathe upon it. At the instant the flim of moisture was passing off, I was surprised to be able to see all the lines which were ruled, one hundred to the inch, with the greatest distinctness. I then carefully filled the lines with graphite, when they were, after the closest inspection, found to be as fine as any I have ever ruled. According to the nearest measurement I could make, their width was about one-sixth of a mikron. Repeated observations gave in every case satisfactory evidence of visibility. In order to ascertain what effect the thickness of the glass might have upon the visibility, the cover-glass was lightly cemented to a glass slide with gutta-percha, when it was found that the lines were by no means as distinctly visible as before. The cover was then removed, when the original observation was easily confirmed. The lines of this plate were readily seen by Prof. Pickering and by several assistants connected with the observatory. Unfortunately the glass was broken in an attempt to mount it upon a brass slide.

While it is a simple matter to rule lines which are easily visible by the unaided eye, especially in sunlight, having a width not exceeding one-fifty thousandth of an inch, I have never since succeeded in obtaining a plate quite as good as the one described. Clearly the ruling crystal has been broken off before this particular plate was ruled, and, as often happens, a minute and delicate crystal remained, which produced the lines which were really traced. In the course of subsequent experiments I found that while the visibility was increased by the flim of moisture, exceedingly fine lines could be seen without this aid to vision when the proper angles of inclination to the source of light are obtained. To get the best results the ruled surface should have an angle of about 15 degrees with the source of light and the lines themselves should have nearly the same angle of inclination. Everything depends upon getting the exact angles of inclination required. More striking results are obtained by sunlight than by artificial light. Highly polished

metals, especially tempered steel and iridium, yielded better results than glass. I will not undertake to say how fine lines traced upon metal can be seen, but I suspect that the limit of naked-eye visibility is far beyond the capacity of ruling. I have a plate of highly polished and nearly pure iridium upon which there are traced a series of lines which are discernible by the eye in sunlight, but which I have never yet been able to see under the microscope by direct light. Yet these lines are easily seen with a low power objective under certain conditions.

I do not propose to offer any theory to account for the facts which I have observed, not even the one which would naturally be the one first suggested,—viz., that of visibility by reflection. I admit that the apparent width of the lines would be increased if the real and reflected lines could be seen side by side. It can be easily shown that the lines in one of the accompanying plates are visible under conditions in which it is impossible for reflection to take place. For the present I content myself with stating the facts of observation illustrated by the ruled plates by which these observations can be repeated.

I close this paper with the suggestion that the increase in the efficiency of the microscope will probably come from the better manipulation of the light under which an object is viewed. At present the unaided eye is a not very unequal competitor of the microscope in the matter of simple vision. In fact, there are certain phenomena connectd with this question which can be better studied by the unaided eye than under the microscope. I believe it to be possible to see under the action of sunlight what can not be seen under any objective. There has been produced upon my ruling-machine, upon a polished surface of tempered steel, a band of ten thousand lines, covering a space of four inches. I have tested the equality of the spacing for aliquot parts of a revolution of the screw in every possible way by direct measurement. Other observers have done the same thing. I can hardly be wrong in the assertion that the spaces indicated by even tenths of a revolution are exactly equal as far as any tests of direct measurement can be applied. Yet, by holding this bar in a certain position with respect to the source of light, the limits of each revolution of the screw can be distinctly

seen. These waves of light and shade indicate an error which can be seen by the unaided eye, but which can not be measured with certainty.

Finally, if the visibility of ruled lines is so erroneously increased by the position which they occupy with respect to the source of light, why may not the visibility under the microscope be increased in nearly the same proportion by some mechanical device which shall enable the observer to find *exactly* the proper angle of inclination at which the light should be thrown upon the object in order to secure the best possible results ?